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date; no disturbance of ocular muscles; fundus of both eyes normal. Regarding the knee-jerk the attending physician makes the somewhat anomalous statement, "the knee-jerk is normal, or rather exaggerated on the left side and nearly absent on the right, but there is not the slightest unsteadiness in his gait, nor any want of power of equilibration on making him stand with eyes shut and feet close together." When last seen by Bramwell in October, 1887, he stated that with occasional momentary "fits of abstraction," which were gradually becoming less and less frequent, he felt perfectly well. "His memory is, he says, quite good, his drawing is better than it was for years before his illness commenced; and for the past five years, he has had absolutely no symptoms of mental derangement whatever. Some physical evidences of disease still, however, remain. His speech is much thicker than it was before his illness; his knee-jerk, as tested through the trousers, seemed absent in both legs, and the attacks of *petit mal*, though steadily diminishing in frequency still continue. There is no longer any evidence of motor impairment. I did not on this occasion detect any twitchings or tremors in the tongue, lips or facial muscles; the pupils were equal and responded briskly to light and accommodation. The patient still has a somewhat heavy, stolid expression, which is probably natural to him; his memory and intelligence seemed active, and, so far as I could judge, in every way natural." After his recovery in January, 1883, he made frequent mistakes in writing, missing out letters and words, and using wrong letters and words; these mistakes became less frequent, and later were rarely observed. Up to the time of Bramwell's report, December 1889, he had continued well.

Dr. Bramwell's case is certainly of great interest, and its future history will be eagerly watched for. It is, however, somewhat odd that Bramwell should ascribe the improvement entirely to the anti-syphilitic treatment, and should not consider the possible effect of the shock following the bursting of the abscess with the loss of blood. Up to the time of this shock no improvement under treatment had been noticed, and indeed the patient was confessedly growing worse. The similarity to Dr. Claye Shaw's case (below) where improvement followed trephining immediately occurs to one. Neither does Dr. Bramwell suggest the possibility that the patient is only in a remission, but to him the case has been "cured" by anti-syphilitic treatment, although he admits that thickness of speech, attacks of *petit mal*, absent knee-jerk, and a heavy, stolid expression are still present. The history of the case at present extends over but eight years, and to the reviewer's mind it is to be regretted that Dr. Bramwell has not qualified somewhat his claims for a cure.

SURGICAL TREATMENT.

The Surgical Treatment of General Paralysis. T. CLAYE SHAW. British Medical Journal, 1889, II, 1090.

Is General Paralysis of the Insane a Curable Disease? GEORGE REVINGTON, M. D. Ibid, 1889, II, 1187.

The Surgical Treatment of General Paralysis. H. CRIPPS. Ibid, 1889, II, 1215.

The Surgical Treatment of Intra-Cranial Fluid Pressure. J. BATTY TUKE, M. D. Ibid, 1890, I, 8.

The Surgical Treatment of General Paralysis. R. P. SMITH, M. D. Ibid, 1890, I, 11.

The Surgical Treatment of General Paralysis of the Insane. GEORGE REVINGTON, M. D. Ibid, 1890, I, 749.

Considerable interest has been aroused by a discussion that has been carried on in the *British Medical Journal* over the question of the advisability of surgical interference in General Paralysis, with the view to relieving the symptoms alleged to be due to increased fluid pressure.

To Dr. T. C. Shaw belongs the credit, if such there be, of making the first attempt to alter the course of the disease, (Dr. Shaw would claim much more), by surgical means. To his mind the pathological appearances in General Paralysis point to an irritative, probably inflammatory, process in the upper layers of the convolutions. The theory of the operation was that by producing an alteration in the existing state of the morbid process, a new and nutritive process might be set up. On the theory of nerve stretching he proposed to stretch the brain by giving it more space in which to expand, by allowing it to relieve itself of the increased arterial pressure, shown by the sphygmograph to be one of the early conditions of General Paralysis. Trephining, according to Shaw, must therefore relieve the later symptoms, due directly to fluid pressure, such as the paralyses, attacks of stupidity, and loss of power of swallowing. Dr. Ferrier agreed that the patient was rapidly approaching a condition of dementia, and that trephining offered the only hope of relief. Patient was in the second stage, with delusions of grandeur, affection of speech, exaggerated reflexes, unsteady gait and retention of urine. Convulsive attacks followed, with short periods of loss of sensation, increased difficulty in swallowing and talking, and rapidly approaching dementia. Trephined on right side of skull, over central sulcus, two inches outside longitudinal fissure. The operation resulted in such improvement that it was proposed to discharge the patient. Shaw states that he did not expect any improvement in the bulbar symptoms, although even here his condition was thought to be better, as he swallowed and spoke more easily; he became less optimistic, and the epileptoid attacks ceased. Shaw considers that the operation was justified by success, although the prominent bulbar symptoms remained.

Mr. Cripps, who performed the operation, speaks less positively of the pathology of the disease. Of the post-mortem conditions, diminution of brain substance and increase of fluid, there is to his mind no absolute evidence to show whether the brain atrophies from pressure of the fluid, or whether the fluid collects on account of the shrinking of the brain. The operation was performed on the former hypothesis, and the extreme manner in which the dura bulged into the wound from the tension of the fluid beneath is considered by the operator to show that the former hypothesis was the correct one. By removing a portion of the bone, with the corresponding meninges of the brain, not only would the existing fluid be let out, by opening the water-proof brain coverings, but a permanent contact would be established between the secreted fluid and the under surface of the scalp-flap, to the absorbents of which was entrusted its future removal.

On these surgical procedures, from theoretical considerations, very obvious and just criticisms are made by Revington. To justify a new and serious surgical operation three conditions should be observed: (1) The theory of the pathological process to be remedied should be unsalable; (2) the mechanical means adopted should be clearly competent to produce the result; (3) the resulting improvement should be the indubitable consequence of the procedure adopted. These conditions have not been even partially satisfied. It is extremely doubtful that the "paralyses, attacks of stupidity, loss of power of talking and swallowing," are due to fluid pressure, but the probabilities are all the other way, that the excess of fluid is a secondary and compensatory process. Further, it is not easy to see how pressure due to fluid in the convolutions could be relieved by a hole in the skull. Finally, the symptoms which Shaw regards as cured by the operation are symptoms which frequently disappear of their own accord. It is extremely probable, therefore, that the favorable symptoms were in no way the result of the treatment except in so far as an issue in the neck might have operated favorably. To Revington, curing General Paralysis, "with no

marked improvement in the bulbar symptoms," is like curing paraplegia without restoring the power of walking, and the permanence of the bulbar symptoms is a curious commentary on the assertion that the loss of power of swallowing, talking, etc., are directly due to fluid pressure.

Dr. Batty Tuke's case appears to have been further advanced than Shaw's. Patient was trephined a little above and in front of the parietal eminence on both sides. No sign of bulging of the brain into the hole was observed; the buttons of bone were not returned. In the first case it was held that the bulging of the brain into the trephine hole upheld the pressure theory; the second operation was performed on the same theory, but the absence of bulging in this case is not cited as in any way militating against the theory. It was thought that for five days after the second operation the pupils reacted better to light, and the intellect of the patient was distinctly clearer, and "he was evidently more sane than previously." After five days the old symptoms returned. He was removed to Morningside, and Dr. Clouston thought the operation might possibly have modified the symptoms. When last seen by Tuke, the patient was much demented, with well marked ataxia, and showed progressive symptoms of a downward tendency. Tuke felt that the results justified the operation.

Dr. Smith's paper is a criticism of Dr. Shaw's case; he deprecates that it should not have been made to appear that it is no new thing for general paralytics to so far improve that they have to be discharged "recovered," because there are not sufficient symptoms to warrant their legal detention, though the physician may be convinced that they will eventually relapse and end fatally, and he quotes several such cases.

He does not admit that the operation was justified by success, and doubts whether the trephining had any more to do with the improvement than any other injury might have done. Smith quotes Mickle, where in several instances "recovery," or very long periods of remission have supervened on accidents, violent injuries, or diseases of such a kind as to produce revulsive effects.

Revington's criticisms in Tuke's case appear to have ended the controversy, for a time at least. Tuke's description of the case, according to Revington, shows that the disease made rapid progress, and the patient seems to be worse off than many a ten-month paralytic with a sound skull, so it is not at all sure that the operation may not have "modified symptoms" in a way not anticipated by its originators, and he must be a sanguine man who would take the improvement noticed during five days after the operation as worth much. It is, however, on the ground of the false pathological theory that the two operations are chiefly to be criticised. The pressure-theory, according to Tuke, makes it certain that obstructed lymph can make its way but imperfectly by natural channels to the pia-matral space, and becomes diffused through the tissues, injuring and displacing cell and fibre, and impairing their functional activity, while the operation, by permitting a healthy action of the lymphatics and blood vessels, stays the process of sclerosis. Revington is probably right in saying that the entire mass of pathological evidence is absolutely contradictory of such a theory as this, and that the typical cell degenerations found in General Paralysis are not such as may be expected to follow simple excess of fluid pressure, but is rather (citing Bevan Lewis) a true degeneration, due to acute interstitial anomalies, with no notable differences between the changes through which the cells pass and those in senile atrophy, and that there is no excess of fluid in the first stage, while the second stage is one of extraordinary development of the lymph connective system of the brain, with a parallel degeneration and disappearance of nerve elements, the axis cylinders of which are denuded. In the first stage, then, the only stage

in which an operation would be justifiable, there is no excess of fluid, and in no stage is the excess of fluid of more than secondary importance. We may, therefore, conclude with Revington that the results in Dr. Tuke's operation are quite insignificant; that the results of Dr. Shaw's operation are most probably due to a remission; that the pathology upon which the operations were founded is opposed to all the best knowledge on the subject, and that the collation of two cases warrants nothing so clearly as the opinion that little good can be expected from the operation of trephining in General Paralysis.

III.—EXPERIMENTAL.

Ueber das Erkennen der Schallrichtung. J. VON KRIES. *Zeitschrift für Psychologie.* Bd. I (1890), H. 4—5, S. 235.

The ear perceives the direction and to some extent the distance from which sounds reach it; is this a judgment based upon the difference of the primary sensations reaching the two ears, or is it the more or less simple sensation of a special organ? The results of a very few rough trials will convince any one that right and left location is much more exact than location in any direction in the median plane; and this fits very well with the theory that sound is located by the difference of intensity of sensation in the two ears, because in the last case the two ears are always equally stimulated. Such recognition of direction in the median plane as is actually found has then to be explained by changes in the quality or intensity of the sound as its point of production is shifted about the head in that plane. Even this ought to fail, however, in the case of sounds with the quality and intensity of which the experimentee is unfamiliar, and this is the point which v. Kries set himself to try. The sounds of a telephone, of whistles, of bits of wood or metal snapped together—all of them variable to a certain degree in quality and intensity at will—were produced at different points in the median plane. The experimentee had to decide between but two possible locations (*e. g.* before or behind); he was kept in ignorance of the correctness of his answers, and in general every precaution was taken to prevent his learning the character of the sounds so as to be able to judge by that of their positions—as also to prevent a betrayal in any other way of their location. The experiments were made largely upon v. Kries and his assistant, but in addition some 22 students were also tested. In the first experiments (comparing forward-and-upward with backward-and-upward, telephone click as stimulus) the location was very uncertain; the next set (forward-and-upward as compared with forward-and-downward, whistle stimulus) gave much better results, as did also the next (upward compared with backward, stimulus by snapping coin). In other experiments where a continuous noise was produced by the telephone for 0.5—1 sec., the location was still more exact; *e. g.*, v. Kries, with before and behind as directions, made 39 correct judgments out of 44 trials, 4 times was in doubt, and only once answered incorrectly. On the 22 students very few trials were made, only five or six on each (comparing backward with forward) so as to exclude still further the possibility of learning the sounds. On the whole, the results of these experiments did not show exactness of location, a fact not much to be wondered at, perhaps, in so few experiments, but did show very great individual differences in this power. One student answered the first six times correctly, in an additional 30 trials was right 29 times, and showed an almost equally exceptional power in recognizing other directions.

For the detail of the experiments, as for points of incidental interest, the reader is referred to the original. Among others, however, may be mentioned the following: The occurrence of constant tendencies to say